

J. F. Portis 11 rue Cernuschi Paris 17

20th August '47

Dear Dr. Lederberg,

I had read in due time the paper who bore Dr Tatum's signature as well as your own - Nature 19 Oct '46 - and I have just been reading your recent work on the same subject (J. Bact. June '47).

I wish to point out first of all that in my opinion, your work is not only excellent but, what is more important still perhaps, that it is done in a direction that shall prove in the future most fruitful in many and quite different fields.

Secondly, I wish to call your attention on one point: you have been doing experimental work to see if you could induce some modification in bacterial chemism by using 'transforming substances' (taking into consideration O.T. Avery & M. McCarty's researches). For this purpose you used centrifugation & filtration so that you could separate the products offered by bacteria, from the microorganisms themselves. With these substances you undertook your experiments, that turned out to be negative.

Now, I might have told you beforehand that you had not a chance in a million of obtaining a positive result, owing to the manner you prepared your transforming substance. I believe you were right in pointing out that some subtle change may have occurred in the medium preventing the substance from being active.

Owing to the experiments I have undertaken on a quite diff.

-fent and still closely connected line with your own work, I think that the way you prepare your transforming substance is not the best for the purpose you had in mind; i.e. I believe there might be ~~and~~ an other and possibly a better way.

Now, Mr. Morgan: I am not in the least pretending that with the alternative preparation you should obtain a positive result. I am saying nothing of the sort, having not gone through the same sequence of experiments as you have. But what I mean to say is that instead of having one chance on a million of obtaining a positive result, you might have something like, let's say, one chance on a hundred. This of course, does not make the odds so very good, but at any rate it does not make them as bad as otherwise. That's all.

If I insist on this position, it is because the experiment you undertake is quite important and therefore it seems to me it should be done in such a way so as to place oneself in the best conditions.

It is possible that I shall be going to the States either next autumn or during the winter. I suppose a talk would be the easiest and fastest way of explaining my idea on this special point.

Should such a scheme receive your agreement? If so, would you please let me know where I might eventually meet you?

Once again, I do appreciate your work; particularly, know-

ing how very young you still are.

With kind regards,

Believe me, sincerely yours

J. F. Don'tis

ansd Aug 26 1947